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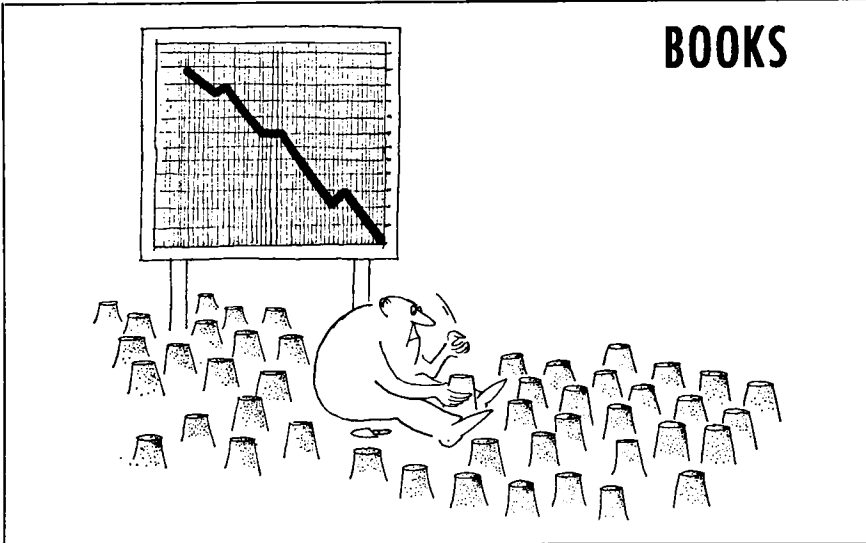


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BOOKS



THE VIRUS HOUSE: THE GERMAN ATOMIC BOMB PROJECT

EUGENE RABINOWITCH

The publication of The German Atomic Bomb by David Irving (New York: Simon and Schuster, 1968. Pp. 329. \$6.95) has promoted wide discussion in Germany and the United States.

Eugene Rabinowitch reviews the book in the first article which appears below. The Bulletin then reprints a review by Werner Heisenberg which appeared in the December 1967 issue of Frankfurter Allgemeine Zeitung; Professor Heisenberg was head of the "Virus-Haus" in Berlin-Dahlem. The last essay is by Hans Suess, professor of chemistry at the University of California, San Diego. Professor Suess appears in the book as one of the German scientists during the time under discussion; he offers his personal recollections of German science during World War II.

The story of the American atomic energy project during the Second World War was told, in considerable detail, immediately after the first use of the bomb in Hiroshima, in the so-called Smyth Report. Extensive details were provided in the official history of the project by David Hawkins (*Manhattan District History: Project 4*), published in 1945. The British told their side of the story a few

months after the Smyth Report. No full report on the Soviet wartime development was ever released; but many details were revealed in biographies of Igor Kurchatov, who had headed Soviet developments until his death in 1960 (*Bulletin*, December 1967). Still missing is a complete story of the German atomic energy efforts. Professor Goudsmit, scientific head of the American investigative team that went into Germany at the end of the war (together with, and sometimes ahead of, the troops), described his findings. The Alsos (Grove) mission removed from Germany not only its stocks of uranium and the most important German scientists involved in the project, but also all documents, reports, diaries, etc. on which the mission could lay its hands at Hechingen, Haigerloch, Munich, and other places to which German nuclear research had been evacuated in the last years of war. These documents remained stored at Oak Ridge and other U.S. archives until the British journalist, David Irving, located them and obtained permission to peruse them. He was given assistance by many German and other officials, with the notable exception of British authori-

ties who refused him access to the tapes of conversations between the ten German scientists (Heisenberg, Hahn, von Weizsäcker, and others), who were held incommunicado in a farmhouse in England when the first American bomb was dropped on Hiroshima.

The results of Irving's thorough studies of these documents are contained in an obviously reliable, blow-by-blow account of the German research, from its promising beginning in 1939 to a peak in 1942, and its stagnation and ultimate demise in 1943-45.

The account reveals that the worries of the American scientists (primarily of the European refugees among them) about a possible German success in producing the first atom bomb (which caused them to urge American efforts in this direction) were not exaggerated. In fact, until 1942 the Germans were ahead of the British and the Americans, both in the knowledge of essential scientific facts and in the scope of their experiments. The Einstein letter to Roosevelt on August 2, 1939, which became the starting point of the American project, was preceded, on April 24, 1939, by a letter to the German War Office from the Hamburg physical chemist, Paul Harteck, and his co-worker, W. Groth, suggesting the possibility of making nuclear explosives and pointing out the great advantage which would accrue to the nation having the first success in this enterprise.

The developments that followed were at first more rapid in Germany than in the United States or Britain, both in the reactor development and in the separation of uranium isotopes. These efforts were supported by the thousands of tons of uranium ore which fell into German hands when Belgium was overrun in May 1940, and by the German control over the only then-existing heavy water production plant in the world, occupied a month earlier in the German invasion of Norway. Assemblies of uranium or uranium oxide, in the form of plates or cubes, submerged in heavy water, were soon put together in the so-called Virus House in the Kaiser Wilhelm Institute complex in Berlin-Dahlem under Heisenberg, and in Leune-Merseburg works under Har-

teck, ahead of the efforts of Fermi, Szilard, and Zinn in New York.

In 1942, however, the German project began to slow down, while the American one was rapidly expanding. The isotope separation research of Clusius, based on thermal diffusion, was unsuccessful, and no attempts were made to achieve isotope separation by effusion, was successfully carried out in America by Urey and co-workers in New York on the laboratory scale, and later in Oak Ridge on the production scale.

A fateful error in the determination of the neutron cross-section of carbon by Bothe stopped the Germans from experimenting with graphite as moderator in reactor assemblies. Instead, they put all reliance on heavy water as moderator; but just when the supply of heavy water from the Norwegian plant at Rjukan became ample, a decisive blow was inflicted by Norwegian parachutists, who blew up essential parts of this plant in February 1943. Attempts to resume heavy water production in Germany petered out, largely because of general disruption caused by allied air raids.

The attempts at electromagnetic separation of isotopes by Manfred von Ardenne in his underground laboratory on the outskirts of Berlin continued successfully, but its output was not large enough for the building of a U-235 bomb. Consolidation of all heavy water supplies in a single reactor project, under Heisenberg, twice evacuated and ending in a cavern near Haigerloch in Württemberg, permitted construction of an almost critical assembly. The work on this reactor was continued until the Allied troops were within a few miles. The German scientists nourished to the last minute the hope that if they succeeded in producing a self-sustaining nuclear reaction, they would give German science and German industry a tremendous boost after the clearly imminent military defeat. They did not suspect that, in the spring of 1945, Fermi's first self-sustaining nuclear reactor in Chicago was already over two years old, and giant plutonium-producing reactors were in full operation at Hanford. They were stunned, and at first couldn't believe it, when the explosion at Hiroshima revealed that the American development had progressed far beyond their own.

Another reason for the German failure, in addition to Bothe's error and the limited supply of heavy water, was the belief of German military leadership in rapid victory, and its consequent lack of interest in a project unlikely to pay military dividends within a year or two. This reluctance became particularly strong under conditions of economic disruption and manpower stringency which developed when the war continued into its third and fourth years. In Irving's interpretation, which American scientists who had worked on the Manhattan Project are unlikely to accept, another reason was that the American development was taken in hand by energetic military leadership, accustomed to operation on a large scale, while the German project remained to the end in the hands of scientists. They quarreled among themselves over limited supplies of uranium and heavy water, espoused different ideas concerning what could be achieved, and toward the end were interested primarily in the progress of nuclear physics under the guise of war research. Irving is particularly skeptical about the theoretical physicists, such as Heisenberg, von Weizsäcker, and others, compared to the more practical chemist such as Harteck, and inventors, such as Manfred von Ardenne. The German physicists surprised the military and political leaders by the modesty of their financial requests, which suggested to the leadership that they had little faith in their project. Paraphrasing Lloyd George's famous statement, Irving suggests that science in wartime is too important to be left to scientists!

While many of the American physicists on the Manhattan Project recognized the great organizational talent and drive of General Groves and his team of Army engineers, most of them complained that the military leadership had slowed down the development by its obsession with security (which proved ultimately ineffective). This was particularly true of the compartmentalization of research, enforced by General Groves, which impeded rapid exchange of information and ideas between the different laboratories. Only at Los Alamos, where the bomb itself was developed, did Oppenheimer succeed in preventing compartmentalization, permitting free discussion among all

members of the laboratory. The original drive and leadership on the Manhattan Project was provided not by experimentalists or engineers, but by theoretical physicists—Wigner, Oppenheimer, Szilard, Fermi (the last a unique master of both theory and experiment). (Physical chemists, such as Seaborg, Urey, and Spedding, also did play a very important role, particularly in the separation of plutonium from uranium.) While it is true that the gap between scientists on the one hand and industrial and military technicians on the other was wider in Germany than in America, few American scientists will "buy" Irving's conclusion that what was missing in the German project was efficient military leadership.

One last aspect of the German failure to develop an atom bomb in wartime has attracted considerable attention: alleged lack of wholehearted support of their government by German scientists, and consequent reluctance to provide Hitler's regime with a terrible new weapon. This was asserted first by Robert Jungk in his bestseller, *Brighter Than a Thousand Suns*, and repeated by many German scientists, including Dr. Suess, a former co-worker of Harteck, in his criticism of the Irving book reprinted on page 36 of this issue. These doubts have caused German scientists to welcome the conclusion that atom bombs could not be developed during the war, thus avoiding the need to make a decision on whether or not to produce a bomb for Hitler, and to concentrate instead on the development of nuclear reactors as postwar sources of industrial energy. They caused Heisenberg to hint, in a conversation with Niels Bohr in Copenhagen in 1941, that if the Allied scientists would renounce the development of the atom bomb for the American government, this would strengthen German scientists in refraining from making one for the German government. However, the hint (if any) was so vague that it was not understood by Bohr, who soon after this conversation escaped from Denmark and as "Mr. Baker" joined the Los Alamos laboratory. Suess points out that Irving's book, although reliable and objective in telling the documentary story of the German project, could not convey properly the psychological situ-

ation in wartime Germany—the mistrust between scientists and the political leadership, and even between individual scientists, engendered by an all-pervasive totalitarian and intellectually incompetent dictatorship, and the consequent tendency to avoid responsibility for pushing the project ahead.

Undoubtedly, distrust and lingering doubts about the possible consequences of Hitler's Germany acquiring nuclear weapons did exist in German scientific circles, but it remained vague, inchoate, and altogether inadequate to absolve the German scientific community of participation in the great crime against humanity perpetrated in the name of the German nation by the fanatics of racism. Some German scientists were at first as eager to point out to Hitler's War Office the possible advantages of first acquisition of nuclear weapons as were Einstein, Szilard, and Wigner in America. In the minds of many, perhaps most, German scientists, the distrust of the Nazi regime was tempered by a vague hope that this regime represented only a temporary ugly face of a German national renaissance. The danger of military defeat to the future of the German nation overwhelmed, in their minds, the concern with the crimes of the then holders of national authority. For "unpolitical" German intellectuals, including some outstanding scientists, the Nazi movement, with all its crimes, represented a national revolution against a decaying, international, urban civilization, and a return to traditional moral values of the German race.

It was typical of German intellectuals that they avoided simple, pragmatic judgment of political realities by resorting to vague, general philosophizing, avoiding in this way the simple moral choice which confronted them in the face of Hitler's barbaric racism. Only a few, such as the great theoretical physicist, Max von Laue, the faithful friend of Einstein, escaped such philosophizing. Heisenberg was not among them; and some lesser minds saw in the expulsion of Jewish scholars a chance for faster advancement in the overcrowded German academic hierarchy.

When the war dragged on, and the likelihood of German defeat loomed more and more ominously for all who

retained a modicum of rationality, the scruples of the leading German physicists became stronger, and the alibi of developing not an atom bomb, but a postwar reactor, actually became reassuring.

Jungk's comparison of the hesitancy of leading German scientists to the eagerness of American scientists to push ahead the atomic bomb project, cannot be accepted as valid.

In the face of the fundamental difference between the (relatively) civilized governments of the West and

the cruel and barbarian regime in power in Germany, only a blame of a different order of magnitude can be laid at the feet of the American scientists. It lies in their failure to readjust their thinking when the danger of the German atom bomb was dispelled in 1945, and to rally a more effective opposition to the use of the American bomb on Japanese cities, attempted in the so-called "Franck Report" of June 1945. This report was not supported by the leading scientists of the Manhattan Project.

THE THIRD REICH AND THE ATOMIC BOMB

WERNER HEISENBERG

Virus-Haus was the cover name of a small laboratory in Berlin-Dahlem in which, at the beginning of World War II, the first German experimental research work on the utilization of atomic energy took place. David Irving uses this name as the title for a book describing in detail the German work in the field of atomic energy during the war. Around this work various legends were spun in later years, ranging from a suspicion that the atom bomb dropped on Hiroshima was a German-made bomb found by the Americans in the cellars of Haigerloch or Nordhausen, to the contrary assertion that German physicists committed high treason, keeping the knowledge of the atom bomb a secret from the Führer. One has to be grateful to the British historian, David Irving, for the trouble he has taken in devoting an extensive and time-consuming study to the sources of these legends and to a historically correct description of the events of that time. In this he has had the full support of German authorities and considerable support from Allied officials—considerable, but not complete. The informative tapes, recorded by means of secret wire-tapping devices, the conversations between the German nuclear physicists—held in 1945 as prisoners-of-war in England—after they were informed of the first atomic bomb drop over Hiroshima, have not been made available to him. In other respects, the book is supported by a much more profuse supply of sources, documents, pictures,

and conversations than was available even to the German authors of a report that appeared in 1946. Irving's description is fluently written and easily readable; many pictures enliven the text and memories of the atmosphere of that time. It is satisfying that Irving's investigation confirms the German report in all important points; beyond that it supplements and completes this report, presenting a clear and reasonably unbroken picture of the facts.

What are the most important facts? In the years 1939–42 the scientific basis for the technical application of atomic energy was laid in Germany, as well as in America. We learn from Irving that a few months after the beginning of the war, theoretical research had led to the conclusion that atomic reactors could probably be constructed by using, in addition to fuel made from natural uranium, deuterium oxide (heavy water), or pure graphite as a moderator to slow down fission neutrons. Furthermore, in such energy-liberating reactors a product will be formed from uranium (later named plutonium in America) that could be used as an explosive in atom bombs; and finally, that atom bombs could be built on the basis of a chain reaction with fast neutrons, if uranium enriched in the isotope 235 (the enrichment is technically an exceptionally difficult problem) or the just mentioned combustion product of the atomic reactor (plutonium), were to be used as an explosive. The experi-

mental research work carried out during the first years of the war gave these conjectures a solid foundation. According to Irving's careful description, the German physicists knew, in the summer of 1942, that the construction of atomic reactors from ordinary uranium and deuterium oxide (heavy water) was in fact possible. Incorrect measurement of the neutron absorption cross section of pure graphite did prevent the conclusion that this material, too, would be suited for the construction of reactors. Nevertheless, at that time, as Irving justly stresses, a method of constructing atom bombs was known in principle. The explosive plutonium could be produced in reactors with heavy water. A technically useful way of manufacturing the other nuclear explosive, uranium 235, had not been developed at that time. The German authorities responsible for armaments were, as Irving correctly reports, informed of these results at a conference in the Harnack-Haus in Berlin-Dahlem on June 4, 1942. However, no serious attempt to construct an atom bomb was ordered by German authorities after the Conference.

Subsequent experimental work, which Irving tells us never came to a complete standstill, amounted to running in place. At the end of the war, the results did not go much beyond those of 1942. The author records this fact without finding a sufficient explanation for it. The destruction of the only larger plant for the production of heavy water in Rjukan, Norway, first by a sabotage group and later by the British Air Force, hardly provides a sufficient explanation. No serious protection of this plant against attacks had been provided; also production of heavy water is not a very difficult technical problem, and it could have been undertaken in Germany.

In reality, the negative decision made by German authorities in the summer of 1942, which Irving does not adequately stress, was quite understandable and logical. At that time the war situation was already too tense for long-term technical projects. An order is supposed to have been issued prohibiting technical developments which would require more than half a year for completion. This situation spared the German physicists the decision whether to plead for an at-

tempt to produce atom bombs; they knew, on the basis of their technical experience, that such an attempt could not lead to success in less than three or four years. An attempt of this sort would have undoubtedly hastened German defeat, because the extensive manpower and materials necessary for it would have to be borrowed from other sources, thereby lessening the production of tanks and airplanes. Actually, in America, too, despite its much greater scientific and technical potential, in a country where the armaments industry could work without any disturbance from enemy air attacks, the production of atom bombs was completed only after the end of the European war. This fact was given too little consideration by Irving.

Indeed, at this point a weakness of Irving's book appears which is perhaps unavoidable. Wherever it is a question of facts, Irving's report, based on extensive studies of sources, is reliable and exact; but when it is a matter of motives, such documents are no longer sufficient. Here an intimate knowledge of the psychological situation in a totalitarian state at war would be necessary. Irving cannot possess this knowledge; acquiring it would be extremely difficult for a member of a Western democracy. Irving does not always escape the temptation to fill the gaps with stereotype ideas. He describes correctly the rivalries which existed between different German authorities—similar, by the way, to those that existed in America—but he does not recognize sufficiently how deep a mistrust could exist, yes, often has to exist, between human beings in a totalitarian state, even between those who work closely together. Misplaced confidence could be fatal. This some of our friends did in fact experience under the Third Reich. That at times we also inflicted bitter injustice through our distrust only became clear to us after the end of the Third Reich. Of this aspect of the situation Irving obviously knows but little.

By reading Irving's book one becomes aware of a peculiar, not always avoidable, danger connected with the temptation to misinterpret the psychological situation, and to resort to stereotype. When you read the book for the first time an impression can occasionally arise diametrically in contrast with the real factual content

of the book. This is demonstrated by a review that appeared in England, in which the essence of the book was not correctly understood.

In spite of such shortcomings it remains the great merit of the book that it presents the facts accurately and exactly. Whatever one may think about the motives, it remains a fact that a serious attempt to produce atom bombs in Germany was not undertaken, although in principle—but perhaps not in practice—the path to it had been open since 1942. But German physicists did not insist on pursuing, by means of practical measures, a path which could not have led to success during the war.

Whether Irving's book will be sufficient to destroy all legends is doubtful. After a great war, history is written by the victors and legends develop which glorify them! Therefore, in spite of Irving's book, one can predict a long life for the legend of the "race for the atom bomb."

The above was translated from German by Margaret Seckel.

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VIRUS HOUSE: COMMENTS AND REMINISCENCES

HANS E. SUESS

"Germany's Atomic Research and Allied Counter-Measures" is the matter-of-fact subtitle of a book by David Irving which leaves one wondering how to define the very concept of historical truth. It engulfs the events of this period in a strange twilight, though much of the book is lucid and authentic, and long-forgotten details are recalled. Some of the most essential questions, however, are left in the dark, and misconceptions and misunderstandings are frequent.

It takes some time before one realizes why the book is wrong in many basic aspects and yet presents details with vivid truthfulness and unprecedented accuracy: the prime sources for the book have been German war files which, as the author states, "had been lying unused and neglected in a warehouse of the U.S. Atomic Energy Commission at Oak Ridge, Tennessee." Irving undertook the tedious task of piecing together from hundreds of documents a consistent story which, he then felt, was confirmed by numerous personal interviews with people who had lived in Germany during that time, and with many others who knew something about the course of events. For anyone who is familiar, because of personal experience, with the situation in Germany during the last war, this book demonstrates vividly all the tragic errors a historian can make if he derives his conclusions from the official documents of a totalitarian country. Irving was able to draw valid conclusions from German documents concerning scientific knowledge, ideas, and reasoning. But his views on anticipations, perspectives, and motivations are necessarily distorted and tend to correspond to the National-Socialist party line and to the official propaganda. Irving is a victim of the same kind of delusions that, shortly after the war, misled American investigators into believing that official Nazi files were better and more truthful sources of information than the testimony of reliable witnesses in such matters as the political conduct of individuals.

The following comments and additions are based on personal experience

and recollections. They are intended to help the historian reconstruct the prevailing atmosphere in which German atomic research was conducted and to learn what the motivations for Allied counter-measures during World War II might have been.

Scientists of all countries have the reputation of being nonconformists. Their psychology, both as individuals and in crowds, does not follow established rules. This was true, also, for the scientists working in Germany at the beginning of the war. Nothing could be more wrong than the assumption that they were guided by the eighteenth-century principle, attributed to U.S. Navy Captain Stephen Decatur, "My country, right or wrong." The majority of the German scientists were, at that time, far more critical of their government than the average American scientist ever was, or is, of his. As always, curiosity and ambition were the two chief motivations for scientific research, but at the beginning of the war a third was added: the desire to survive.

It is often believed that in the minds of many German scientists a conflict existed between their responsibilities toward humanity in general and their national responsibilities. This, however, was not true. Whether or not individual German scientists would have been willing to work on an atomic bomb for Hitler might be an interesting psychological question, but this question did not arise. As Werner Heisenberg repeatedly pointed out, the majority of the German scientists did not feel the need to make such a decision; they were convinced that their work would not lead to the development of a bomb, or to any other significant contribution to the German war effort. History has shown that this was true. Occasionally some doubt was expressed that the degree of confidence in this conviction was fully justifiable. That this confidence was warranted could be made plain and the many reasons which made the situation obvious to the unbiased observer could be listed. One of these reasons, the low war priority under which the German atomic research was conducted, is discussed below from the perspective of my own experience. There did exist, however, another type of conflict in the minds of many German scientists: the moral dilemma

created by the question of whether it was justifiable even to pretend to study the uses of atomic research for military purposes.

Political or military leaders are usually unable to judge the merits of research projects. This inability provided the opportunity, as some German scientists cynically put it, "to use the war in the services of science." To many scientists this attitude seemed justifiable, as a matter of self-preservation, in their dealings with their political superiors. It did, however, provoke a subconscious feeling of guilt and created the desire to justify and explain their actions to the international scientific community. It is possible that the discussion Werner Heisenberg had with Niels Bohr in October 1951 was instigated by such feelings. In any case, two of the most brilliant men of this century were unable to communicate their ideas to each other; their discussion resulted in a series of regrettable misunderstandings. It seems that even in the most objective and intelligent minds, war mentality can cause a communication barrier which, in some instances, has survived until the present, as can be seen from Irving's book.

This is what Irving says about Heisenberg's attempt in 1941 to discuss the German atomic energy research with Bohr:

"At the end of October, Heisenberg travelled to Denmark to see Professor Niels Bohr, to ask for his advice on the human issue. As Professor P. Jensen aptly put it, Heisenberg, the high priest of German theoretical physics, was going to seek Absolution from his Pope. Heisenberg asked the Danish physicist whether a physicist had the moral right to work on the problems of atomic bombs in wartime. Bohr countered with a question of his own: was the military exploitation of atomic fission possible, in Heisenberg's view, therefore? Heisenberg sadly replied that he now saw that it was. Heisenberg had intended asking Bohr whether he thought it feasible that all the scientists would agree not to direct the efforts of their governments toward the construction of atomic bombs, if he or they could be satisfied that the German physicists were also abstaining from such work. Unfortunately, Niels Bohr does not appear to have formulated his proposal as clearly as this.

"In any event, to Heisenberg's considerable amazement, Bohr replied that military research by physicists was inevitable everywhere and was thus proper too; he refused to be drawn into discussion on the German's proposal, apparently suspecting a German attempt at demolishing the feared American supremacy in nuclear physics, for which the enforced emigration of so many German physicists was largely to blame. Their conversation left a deep legacy of shock in Bohr, and a conviction that Germany was on the threshold of making an uranium bomb."

I feel certain that this account does not give the correct impression of Heisenberg's intentions, but I am certainly not in a position to describe them in an authentic manner. I only hope that my own recollections will help to identify possible misconceptions in Irving's account of the Bohr-Heisenberg discussions in 1941. Irving does not refer to Bohr's meeting, a few months later, with Hans Jensen, now also a Nobel Laureate for his work in theoretical physics.

To the conversation I had with Jomar Brun, the technical director of the Norwegian heavy water plant, early in 1942, shortly before he left Norway for England, Irving refers in the following way:

"Brun's conscience certainly was sorely troubled by the possibility that heavy water might be of military use after all. He had recently learned for the first time of the potential nuclear energy applications from an unguarded reference by Suess to a patent by Professor Joliot. Was this the reason why the Germans were at such pains to increase Vemork's production? Suess's remarks were reported to Tronstad in London: the German physical-chemist had tried to reassure him that the Reich's plans were of a peaceful nature, designed to aid Germany's post-war power economy. Suess had suggested that the development might take many years, but Brun was unconvinced."

Here, I feel that I am well qualified to give a detailed description of what actually happened. Perhaps this description will contribute an understanding of the general psychological situation which prevailed among scientists in German-occupied territory at that time, a situation quite differ-

ent from that inferred by Mr. Irving.

It was not easy for me to make the decision to risk discussing top secret German plans with Brun. Professional diplomats take a dim view of private interference with their work, and an unauthorized discussion of secret matters with a foreign national is considered an act of treason almost everywhere in the world. I was well aware that there might have been microphones in the room, or that Brun might have passed on my information to someone under surveillance by German spies. Moreover, there was a chance that Germany might have won the war and that I would then have been tried for treason on the basis of some captured Allied records.

Brun was primarily troubled by the possibility that the heavy water shipped to Germany might already be contributing to a German victory. Had there not been rumors about a new poison gas? About biological warfare? Brun was visibly relieved when I told him that the matter had no immediate application and that I was convinced that it would take many years, I guessed a minimum of five, before anything practical could result from the research which used the heavy water. "So then this is all *Zukunftsmusik*?" I remember Brun saying. We talked about the possibility of a nuclear chain reaction and the use of heavy water as a neutron moderator. Brun wondered why the Nazis were pursuing this long-range project. I replied that those who believed in a quick victory surely were hoping for its peaceful application after the war, and those who expected a long war were thinking that they must have some knowledge of all the possibilities that might result from such research.

I remember that I was talking freely in my attempt to explain, as convincingly as possible, my personal conviction that heavy water was not going to benefit the German efforts during the current war. I also remember the feeling of fear that everything I said could be held against me during a possible court martial for treason. Brun promised utmost discretion and said that he would carefully consider my safety should he have to pass on the information I had given him. A few weeks later he left for England. His report of my "unguarded" remarks, of peaceful plans of the Reich, etc., was

certainly influenced by this promise.

Many years later, Jomar Brun told me that he never had the slightest doubt that I had given him my honest opinion, to the best of my knowledge. However, he had not been entirely convinced of my competence to judge the situation realistically and he had thought that I might possibly be the victim of wishful thinking, or might have been deliberately misinformed in Germany. This, however, as we shall see, cannot have been the only reason why Brun's report in London did not have any noteworthy effect. Apparently the psychological communication problem that had prevented an understanding between German scientists and Niels Bohr during their discussion in Copenhagen also presented a difficulty for the Norwegians in their dealings with their British and American allies.

It should be easy to draw from existing German documents conclusive evidence on the type of war priority that was assigned in Germany to research on atomic energy. It had never surpassed the stage of "basic research" and its priority was ridiculously low. Probably no one high up in the Nazi hierarchy was willing to assume the responsibility of diverting an appreciable fraction of the industrial capacity to atomic weapons development as was being done in America by the Manhattan Project. In the event of a failure of such a project, Hitler certainly would have had its sponsors executed.

Irving shows very appropriately that the atomic scientists in Germany had mistakenly concluded, from their own inaccurate determinations of crucial nuclear data, that the possibility of getting a nuclear chain reaction going was solely dependent upon the supply of sufficient quantities of heavy water. My official position at that time was that of a scientific advisor to Norsk Hydro on matters of heavy water production. Following a suggestion by Paul Harteck and myself, it was planned to increase the heavy water production in Vemork by installing devices for deuterium exchange between the escaping hydrogen gas and the vapor of refluxing water. It was common knowledge that such exchange can be catalyzed by noble metals such as platinum and palladium. Such metals, however, were not made available for this purpose. Their total

amounts in the territories occupied by Germany were to be used for catalytic syntheses of all kinds, including that of ammonia in the nearby fertilizer plant in Rjukan. A quantity of several hundred grams of palladium would have been necessary in order to increase the heavy water production of the Vemork plant by a factor of two, but we were told that the only material that was both suitable as a catalyst and available for this purpose was a limited amount of nickel. In Hamburg, we had found that metallic nickel, dispersed on pumice stone, would have a sufficient catalytic property and that the effectiveness of such a catalyst was approximately proportional to the square root of the nickel concentration on the surface of the stones. This meant that the larger the volume of pumice stones used, the less nickel would be necessary to accomplish the exchange of deuterium in a given gas volume. It was, therefore, decided that huge drums, filled with pumice stone containing about five per cent of nickel, should be used. This was the result of having spent about six months of my time on tests and experimental measurements. This time could have been saved by making available a few hundred grams of palladium, or some hundred kilograms of nickel. This alone can serve as proof of the low priority assigned to the German atomic energy project.

It is possible that for some time the relationship between the priorities of the nitrogen fertilizer and the heavy water production was not clear to the directors of Norsk Hydro and that this was causing frequent confusion. The relationship became obvious, however, when, following the bombing of the Vemork plant in November 1943, the German Commissioner for Norway ordered the termination of any attempts to revive the heavy water production in order to avoid jeopardizing the production of nitrogen fertilizer.

The only counter-measures against German atomic research were three drastic steps intended to slow down and ultimately prevent this low-priority heavy water production in Norway: (1) a heroic act of sabotage against the high-concentration plant at Vemork in February 1943; (2) the bombing of Vemork in November 1943; and (3) the sinking of the Tinnsjö Ferry in February 1944. The

attack of November 1943 on the little village of Vemork and the hydrogen plant nearby is well described by Irving. It was carried out from Britain by 140 Flying Fortresses of the Third Air Division of the American Eighth Air Force. The Norwegians had good reason to react as described by Irving:

"In London, the Norwegian government-in-exile had been shocked by the USAF raids on Rjukan: there had been no advance consultation with them at all. Nor, for that matter, had SOE (Special Operations Executive) been informed. On December 1, the Norwegians lodged a formal protest-note with the British and American governments, recalling their readiness to provide intelligence on Norwegian industrial installations and to initiate themselves the sabotage of vital war factories with the minimum loss of life and property in Norway. The damage caused by the bombing of Norwegian-Hydro's factories, first at Herøya, and now Rjukan and Vemork, seemed 'out of all proportion to the desired effect.' After the July bombing of Herøya, steps to improve cooperation between the Allied governments had been promised, as the Norwegians protested that they had already evolved plans for sabotaging their country's light-alloy production without crippling the associated fertilizer industry. Despite the promise of closer cooperation, however, the attacks on Vemork and Rjukan were executed without the Norwegians' prior consent, and without their even being informed. 'If,' the note continued, 'the reason for the attack was the necessity of stopping other production than that of fertilizers—for example heavy water—specialized methods of attack would have been more suitable than bombing.' The attack on the Telemark hydroelectric factories left a sour taste in many Norwegian mouths, and a deep-rooted suspicion that the raids were a product of American post-war commercial planning rather than of military necessity.

"It was a month before the British government replied: they rejected both the protest and the Norwegian suggestion that they were best suited to decide upon the fitness of targets for bombing or sabotage. Vemork had been bombed, the British note added, because accurate information had shown that the Germans had increased

security precautions to the extent where sabotage could no longer meet with success. Three weeks later, the Norwegian embassy in Washington was officially informed that the Secretary of Defense had assured the State Department that the most thorough investigation had preceded the bombings. The Norwegians' darkest suspicions seemed to be confirmed when the Americans refused to underwrite Swedish deliveries of the electrical equipment necessary for rebuilding the nonmilitary factories that had been bombed. Sweden sent the material, nevertheless, and within a few months the fertilizer and alloy factories were functioning as before."

Indeed, no satisfactory answer can be found as to why the Allies apparently had been so poorly informed about the state of German atomic research, why no serious attempts had been made to obtain more conclusive information, and why it was found necessary to take such drastic steps as the bombing of a plant owned by Norwegians and sacrificing the lives of numerous Allied soldiers and Norwegian civilians. The most unintelligible action of this kind was the sinking of the Tinnsjö Ferry at a time when German towns were already in ruins and German armies in full retreat. The sad background of this heroic action of sabotage was the following:

Foreign travel has its attractions. In wartime, a visit to a neutral country is a unique experience. Due to currency restrictions, the shopping list of Germans for the short stopover in Sweden, which early in the war had included diamond rings and other jewelry, was reduced in 1943 to such items as replacement buttons for overcoats, new shoelaces, and an occasional can of sardines. Also, the famous barrels of salted herrings were available in Norway. The absence of one of them in the baggage of a British agent on his way to Germany—so goes the story—gave him away. During the last month of 1943 the German war economy was gradually collapsing and no one in his right mind thought seriously of constructing a new heavy water plant in Germany. An order from the German Commissioner for Norway prohibited any attempt to produce heavy water in Norway, and for all those who had had some connection with the Norwegian project

there was no longer any reason to visit that country. The only excuse for one more trip to Norway was the possibility of selecting and securing the contents of electrolyzers in which the deuterium was sufficiently enriched so that it could be processed in Germany at some future time.

Permission for such a trip was granted to a few people, including me. None of us could possibly know that this trip would result in the senseless death of some 20 Norwegians who lost their lives when the Tinnsjö Ferry, *Hydro*, was sunk by Norwegian underground saboteurs. Irving describes this compellingly, but he leaves open the question of why it was still thought "very urgent" in London that the heavy water concentrates be destroyed. Was the sinking of the ferry a matter of exaggerated precaution? Did the sinking result from an administrative blunder, or from professional incompetence? Or was there some reason for this action which has not yet been revealed? It is clear from Irving's account that these are meaningful questions; we should know the answers.

It is difficult to understand why Allied Intelligence should have had so little reliable information regarding the state of German atomic research. A number of scientists associated with the German work, such as Rosbaud, myself, and several others, had volunteered some information. More conclusive information could have been obtained in several obvious ways. In 1942, for example, Brun had suggested to British authorities that they contact me on my way through neutral Sweden at some suitable occasion. No such attempt was made prior to the bombing of Vemork. (See Per Bøhn, *Imi, Norsk Innsats i Kampen om Atomkraft*, 1946, page 62.) None of the research institutes engaged in atomic research in Germany were bombed or attacked in any way; all countermeasures were limited to actions in Norway, although without a detailed knowledge of Germany's plans, heavy water could by no means have been considered a necessary ingredient for the German project. I do not believe that there is any basis for the suspicion, mentioned by Irving, that "the raids were a product of American post-war commercial planning rather than of military necessity." More plausible than a commercial motivation seems

to me to be a political one, namely, the desire to impress atomic scientists working in America and their supporters. In general, it might have been felt that a knowledge of the true state of German atomic research might have adversely affected their enthusiasm for the Manhattan Project.

I have enjoyed discussing all this with Mr. Irving personally, although we could not agree upon the relative values of official documents as compared to the testimony of contemporary witnesses. I appreciate the immense difficulties faced by the historians who wish to reconstruct in an objective manner developments and episodes that have occurred in totalitarian countries. In my opinion, so long as contemporary witnesses remain alive

the true story about German atomic research during the last war can still be prevented from being lost forever.

For anyone, but especially for the readers who have some technical knowledge in the field of atomic sciences, Irving's *Virus House* provides fascinating reading, and a wealth of valuable information as presented in the records. But for an understanding of the psychological situation under which Germany's atomic research was carried out during the last war, and probably also for the understanding of the motivations of Allied countermeasures, the book does not provide a satisfactory background.

The author would like to note that the above reminiscences were written independently of Heisenberg's review.

BEYOND VIETNAM: THE UNITED STATES AND ASIA

By Edwin O. Reischauer. New York: Alfred A. Knopf, Inc. Pp. 242. 1967. \$4.95.

Reviewed by
RICHARD H. SOLOMON

America's distinguished ambassador to Japan from 1961 to 1966 has written a book which finally develops a sense of historical proportion and regional perspective on our involvement in Vietnam. Unlike the mass media, which daily blast us with a soldier's eye view of the war, or numerous recent polemical books and articles which attack the American involvement in this sliver of a country in terms largely isolated from larger security matters, Professor Reischauer has attempted to place the puzzle of the Vietnam war in the surrounding jigsaw map of regional and world problems.

"This is not a book about Vietnam," are his opening words, yet each chapter brings us back to this nagging contemporary problem just as a missing piece in a puzzle is conspicuous by its vacant outline. Reischauer reviews how America's post-World War II reinvolverment in Asian security tasks proceeded according to the largely misleading experience of our successful Cold War aid and military commitments in Europe. He details America's strange and enduring preoccupation with China—an emphasis out of all balance with our "real" economic interests which have continued to reside in Japan. He seeks to redress

our perspective on Asia by mapping the serious imbalance between population and per capita resources in this part of the world, and appropriately identifies the real limits of Chinese power. The author, an expert on Japan, is not being partisan in his efforts to shift America's gaze away from the "Chinese threat" to the region's major power, Japan—an emphasis that seems quite appropriate to this "Chinese" reviewer.

The Vietnam piece in the larger Asian puzzle is most conspicuous when the author relates our present unhappy entanglement to likely future problems. He asserts that, next to a nuclear war, "the greatest danger of the present situation is that . . . the pain of the experience [of Vietnam] may induce Americans to withdraw from Asia into [a] sort of Occident-centered neo-isolationism." His most convincing arguments, paradoxically, are largely supportive of an isolationist conclusion: "We in North America would have the least to worry about [Chinese domination of Asia]. The United States is too rich and strong and so far away. . . . We would not be threatened by the kind of military power Asia could develop." And if America should abandon a "stabilizing role" in Asia, "next to the countries

of that area, Japan would be the chief loser."

The arguments Professor Reischauer marshals in support of an "involve-ment" position are perhaps less convincing because of their conditional and future nature, as opposed to the present costs of the war. He asserts that as part of a unitary world, an Asia which did not evolve toward "peace, stability, and prosperity" through American help would in time present us with regional conflicts which could spread to a world holocaust. Our interest is primarily in Asia's future, in her large population and the capacity of her "industrious people with millennia of high civilization behind them" to affect our lives more directly in 20 or 50 years. And finally, the author sees "simply our own moral imperative to help those who need help."

If the record of the past is any guide to the likely future of American efforts to aid the countries of Asia in their national development, the prospects do not seem bright. The author traces how our present involvement in Vietnam grew from the incremental advance of almost casual decisions until, "bit by bit our commitments in Vietnam built up to the high stakes that now would make our withdrawal and acceptance of defeat so extremely costly." He places the blame for this unwitting involvement on a "grievous misreading of Asian history" by our leaders, and a foreign policymaking process which forces them to "jump from crisis to crisis, thus staying behind our problems as they develop rather than ahead of them." And he notes with frustration America's distorted national priorities, in which billions of dollars are spent on space exploration or intelligence activity, in contrast to a foreign policy formulating process in which "not a hundredth as many people or as much money is devoted to the formulation of the policy itself."

If our governmental instruments of foreign involvement have their limitations, it is the Asian political environment, as so effectively sketched out in this book, which raises the greatest problems for effective American action in the developing world. Our economic wealth, as transmitted through aid programs, is as likely to perpetuate elitist political groups as it

is to filter down through local governmental bureaucracies to reach the people who might effectively use our technology or capital. Our massive military presence can demoralize local elites, who are all too happy to see others bear the burdens of defense while they attend to more pressing matters of political relationships. And the American presence in one country of the underdeveloped world can lead to our entanglement in regional rivalries, or provoke the intervention of great powers doubtful of our motives, thus producing the kinds of confrontations or conflicts we seek to avoid.

Professor Reischauer suggests guidelines by which to resolve these perplexities: "Any regime that is not strong enough to defend itself against its internal enemies probably could not be defended by us either and may not be worth defending anyway"; and he urges the internationalization of our aid efforts.

In overview, however, this book leaves one with a great sense of unease, for the author develops few convincing rational arguments—as opposed to a "gut" imperative to at least make an effort—for continued involvement in Asia, either in terms of our capacity to be effective in an aid role, or in the context of defense against a significant Chinese threat. He asserts that "we should maintain substantial military power in the Western Pacific in

order to deter aggression," yet concludes that even China will "not be a military threat but a more subtle one . . . of incitement to subversion, and of support to insurgency." Yet by Reischauer's own analysis, it is the inappropriateness of our overwhelming military response to a "national" political conflict in Vietnam that is the root of our present dilemma. Does this advice not set us up for future Vietnams?

In sum, *Beyond Vietnam* suggests that the frustrations and puzzlements America faces at this hour are but part of the larger map of political and social change in the underdeveloped countries of Asia; and that given any likely outcome of this present conflict, Americans are certain to face political choices of abstention or intervention in underdeveloped areas where the risks and costs are as great as those we bear today. Professor Reischauer has made an important contribution to our public debate by spelling out the dimensions of this political environment and the paradoxes the United States faces, even if his analysis of the necessity or the capacity for effective American involvement in this part of the world is less than convincing.

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FURTHER COMMENTS ON REISCHAUER AND THE CHOICE ON THE WAR

NATHAN LEITES

While Edwin Reischauer begins his remarkable analysis by indicating that "this is not a book about Vietnam," it is only on that country that I shall follow him here, and only on points or formulations with which I disagree. To them (designated by "R") I shall present alternatives ("A"), without being able to show in these pages why evidence seems to me, on balance, to support the latter. Like Reischauer, "I put forward these . . . views . . . with some diffidence, because I am not an expert either on Vietnam or on military matters" (pages 18-19).

R: It "seems highly probable" to Reischauer that a Vietnam in which

the Communist Party had consolidated the control it had won in the summer of 1945 (rather than, first, losing much of it) would have developed a "relation to China not unlike that of Tito's Yugoslavia towards the Soviet Union"; also because "Ho, like Tito, had had cordial relations with us" (page 30).

A: Mr. Reischauer's alternative history seems highly probable to me, too. But the particular factor quoted last—whatever "cordial" here means—appears to me unrelated both to the conjectured course of events in Asia and to the actual development in Europe.

R: A Vietnam under Communist

control from 1945 on would have been "paying lip service" to China (page 31).

A: Even that is not too probable. There is no traditional dogma in Asian communism affirming the Chinese Party's superiority; and the Vietnamese Party probably feels its record—including a seizure of power in its entire country at a moment when the Chinese still seemed far from that level of success—to be at least as impressive as that of its Northern neighbor against whom no foreigner has intervened since 1945.

R: In 1945 Ho "apparently" expected our continued "friendship" (page 30).

A: Ho does not seriously conceive of "friendship" with the leading state of the non "socialist" world. No evidence is available which would allow us to guess what Ho at the end of World War II expected the course of his relations with Washington to be. He had more to "hope" for in economic aid from us than from China (*ibid*). Our capacity for aid was larger, but what about our intention, actual or predicted by Ho?

R: "The United States . . . gave no indication at Geneva [in 1954] that it would oppose the elections [scheduled for 1956 to decide on the unification of Vietnam]" (page 24).

A: It was hard to believe, and was little believed, that the United States would do anything but "oppose" them if a date were ever set: the majority of the voters (in the North) would have been under totalitarian control. That such elections "would throw the South into the hands of the Vietminh" was not an "assumption" (page 25), but a certainty. To say that "our greatest and most obvious blunder was not to 'support the Geneva agreements' in this regard (page 31) is to assert it to be a grave mistake not to vindicate the totalitarian practice of elections by American 'support.'"

R: The leadership of the Vietnamese Communist Party, ruling the North from the summer of 1954 on, was "outraged" by the "flouting" of the Geneva agreements with regard to elections (page 26).

A: The moment any such leader sensed in himself any attachment to *pacta sunt servanda*, or any depth of outrage about anything an enemy does, he would feel shattered about

his own "degeneration." The leaders in question did not expect Diem, or anybody else attempting to rule from Saigon, to aspire to ceremonial suicide by pressing for elections, about which they themselves were, to borrow Mr. Reischauer's words about these years, "curiously passive" (pages 26-27).

R: A consequence of our "underwriting . . . [Diem's] refusal to go through with the unification of Vietnam" was that the Party's (I shall use this word to designate the Vietnamese Communist Party) "animosity towards us increased" (page 31, my emphasis).

A: Our support of Diem's refusal to transfer the South to the Party's domain by the election mentioned in the Geneva agreements increased the Party's expectation of a conflict with us.

R: One component of the Southern rebellion in the later fifties was "organized" activity by "Southern Vietminh" (page 27).

A: The dominant contribution to the insurgents' organization was and is furnished by the Party, which maintains much centralization despite the country's division, and whose leadership is "Northern" mainly in the sense

that national headquarters are in the North where the Party rules. Much of the "organized" activity by "Southern Vietminh" in the later fifties is likely to have been in conformity with policy set by the Central Committee of the all-national Party.

R: The return to the South of Southern Vietminh who had gone North after the Geneva agreements began after the announcement, in late 1960, that a "National Liberation Front" had been founded in the South (page 27).

A: It began before.

THE TWO REGIMES

R: After the Party's conquest of power in the North its policies produced "agrarian unrest" (page 27).

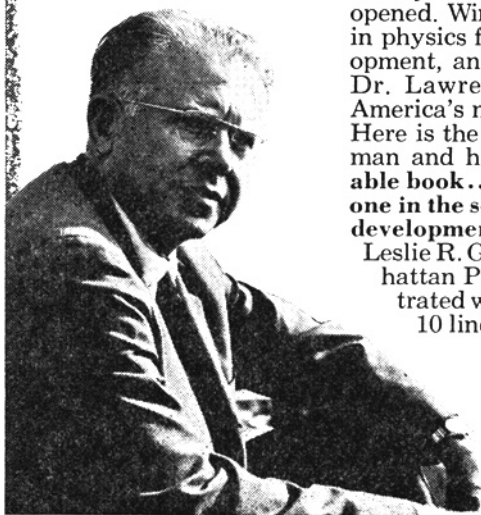
A: At that time the Party destroyed—fully, in an economic sense, and to a considerable extent, in a physical sense—the well-to-do farmers, with a severity at least equaling, say, the "liquidation of the Kulaks as a class" in the Soviet Union of the late twenties.

R: In May 1963 a "serious" Buddhist "uprising" broke out in the South, and in the spring of 1966 a

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"large scale" "uprising" (pages 27-28).

A: These were less than what is ordinarily meant by an uprising.

R: Diem's suppression of Vietminh remnant and of "all open opponents" was "ruthless"; it made "something of a police state" of the South (page 26).

A: If these (proper) words be chosen to describe Diem's conduct, other terms should perhaps be selected to designate the higher level of threats raised and damage inflicted by the Party, before and after conquest of power; and I have missed in Mr. Reischauer's account a sentence which would be a full counterpart to the one quoted. When he speaks of the Party's "dictatorial, oppressive" rule in the North (page 29), the reader little informed about Vietnam may gain the impression that in the author's view North and South are coercing their citizens to about the same degree. Indeed, Mr. Reischauer follows his conjecture that a Vietnam under the Party's control since 1945 "would probably not have been something we would have approved of" with the reminder that "we have not found much we could approve of" in the South either (page 29).

VIETNAMESE BALANCE OF POWER

R: "People usually discuss what Hanoi or Washington might be willing to concede, and what pressures Peking and Moscow may bring." However, "the problem lies with the two . . . protagonists in the war: the supporters of Saigon and . . . the National Liberation Front"; on the one side the Vietcong, and on the other "the South Vietnamese military establishment and a congeries of quarreling religious bodies and political factions," which are, however, "held together . . . by a determination not to fall under Communist rule" (page 8).

A: On the one side there is the Party, in power in the North; in the South, presenting itself and operating largely through its front, the Front.

In the congeries on the other side, the resolve to fight each other seems to surpass whatever "determination" there is to contribute to the prevention of the Party's conquest of power in the South. Vietnam is the only country in the world which remained non-Communist after 1945 solely to the extent to which the threatened or actual presence of foreign soldiers suf-

ficed for this purpose. As Reischauer himself puts it in another context, "all along . . . probably the only . . . alternative to what has happened . . . was to allow Ho . . . to take over the whole of Vietnam" (page 28).

TERMINATION THROUGH NEGOTIATION?

R: The bombing of the North "may so build up" the "hatred and distrust" of "the North Vietnamese" toward the United States that it "increases their determination to go on fighting," reduces their "willingness to negotiate" (page 6).

A: The particular North Vietnamese who, under whatever influence from the people at large, make the decision to negotiate or not, are the leaders of the Party. They are apt to view it as a grievous mistake—as many other less austere politicians also do—not to entertain extreme distrust of enemies at all times, and as a sacred obligation to transform hatred of the enemy into conduct which will reduce as much as feasible losses at his hand, enhance as much as possible advance at his expense. Of course, they do not always succeed in disciplining emotions, but the record shows that they are apt to be as brutal toward their own souls as against enemies.

R: One policy is to "go on fighting on somewhat the present terms, in the hope that in time we could bring about a deescalation . . . and . . . persuade the Vietcong and Hanoi to seek a settlement. . ." (page 9).

A: While it is possible to indicate conditions in which the leadership of the Party might decide to deescalate, it is harder to imagine situations in which they would prefer explicit self-limitation to temporary reduction in activity—unless they came to believe that the former would decisively weaken Saigon (in which case it might not be acceptable to us) or furnish a pretext for an American withdrawal. But the acquisition of such insight by the Party's leaders is obstructed by their horror of yielding beyond present necessity, as well as by their certainty that only pressure works.

R: "I wonder if any sort of agreement will ever be reached until one side or the other recognizes that it faces eventual defeat" (page 8).

A: Particularly then each side may

prefer unadmitted (where feasible) or properly embellished withdrawal to formal renunciation, or (for us) the appearance of "peace with honor" soon to be followed by utter failure.

R: After the American elections of 1968, the Party may be "more inclined" toward a negotiated peace if it is given, in the South, a "tolerable" alternative to an "apparently endless" war (page 18).

A: With dedication, courage, and skill, the Party has been fighting for more than 20 years; there were interruptions, to be sure, but while they lasted, the Party looked toward the resumption of the struggle. Though the trend has been in its favor, there have been lengthy phases of stagnation and regression; the leaders expect them to recur, and have not ceased molding themselves and the cadres to be tenacious in stalemate and adversity.

At least until the early seventies, therefore, the Party is more apt to decide in favor of lying low in the South—as it did from the middle to the later fifties—than to reconvert itself so as to operate in an alien legality, supposing Saigon were to offer it a genuine chance of that kind.

ESCALATION

R: Attacking on the ground in the North "would probably only mean . . . that we would have two guerrilla wars on our hands. . ." (page 10).

A: Worse. Even if we succeeded, with a much enlarged investment cost, in depleting our opponent's armed forces in both parts of the country, induced him to disperse into small units and to avoid encounters with us—if we were as successful as the French in Algeria, 1959-61—the maintenance cost of this result would probably include a protracted continuation of our massive military presence. This kind of forecast made de Gaulle, in late 1960, decide upon "disengagement" from Algeria.

WITHDRAWAL

R: The "counterstrategy" of building a "more tenable defense" (against the expansion of the zone where Communists rule in Asia) in "sounder terrain," such as Thailand, would mean "the further spread of American military power into areas where the Vietnam war had just shown that our

type of military power was . . . ineffective" (page 13).

A: The withdrawal would have been caused not by what Vietnam and, say, Thailand have in common—being in Southeast Asia—but rather by what has made a difference between them for more than three decades: the Vietnamese Party's performance—the level of political and military energy generated by it per capita of cadres and country—is among the highest in the world; that of the Thai Party, insignificant. On the other hand, those opposed to the Party in Bangkok have been less feeble, and less prone to consume their forces in struggles among themselves than has been the case in Saigon.

R: Withdrawing from Vietnam would be "welching" on a commitment.

A: Yes, as to the texts of assurances given. No, with regard to a rarely stressed, but hardly hidden, condition never clearly renounced by Washington: the enterprise of preventing the Party's conquest of power by arms in a country should be a genuinely common enterprise of its anticommunists and their foreign allies. That the internal resistance against the Party might be and might remain feeble was hardly envisaged in the American mainstream until recently. That is the case of Vietnam, familiar today, but unexpected by those who initiated and enlarged American intervention between the mid-fifties and the mid-sixties. Imagine a weird world: a temporarily cohesive, impressively large, reasonably dedicated, and moderately efficient Freedom Front, capital Hué, utilizing much of its human and material resources in its civil war—and insisting on our doing the rest, urging us to destroy what must be, as, to a noticeable extent, the occupied Europeans did a quarter of a century ago—that is the "doctrine" of "containment" applied to Vietnam. At no point has it been resolved, at any level in the United States, that we assume henceforth the obligation to endeavor, by our own force, stopping the Communist Party in a country where the balance of the will and skill to believe and persuade, to coerce and suffer, to kill and die, is decisively in its favor.

Nathan Leites is a professor of political science at the University of Chicago.

CHEMICAL ABSTRACTS SERVICE ANNUAL REPORT TO NSF

EUGENE GARFIELD

In the Spring of 1965, the National Science Foundation (NSF), acting as coordinator for several government agencies, signed a contract with the American Chemical Society (ACS) to develop and test a registry file of chemical compounds and to conduct selected research and development on chemical data handling. The Chemical Society has now reported on the first year of what originally was to be a two-year program costing the taxpayers over \$2 million. Already it appears that consideration is being given to escalating the project in duration and size at a cost of \$20 million.

Whether or not the government continues to fund this project is of vital concern to the entire scientific community—not just to the relatively small number of industrial and government chemical organizations who will make the greatest use of this system when and if it comes into operation.

The project and the report should raise a broad range of questions—political, scientific, and economic—for serious consideration by the scientific community and the public at large.

When the federal government subsidizes a new scientific information activity, the scientific and lay communities should be alert because government subsidies are always accompanied by government controls. The degree of control which the government chooses to exercise at any particular moment may be unpredictable but recent history is replete with instances of ill-advised and even silly restrictions placed on the free flow of scientific information in the name of national security.

If, however, the government claims that it does not intend to control, but merely to support, an information program, the citizen may rightly ask by what authority it grants a particular private organization a virtual monopoly

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oly on a vital source of information. This criticism was anticipated in intragovernmental discussions which preceded the award of this contract to the American Chemical Society. That the particular private organization happens to be a professional society with a tax-free status, will not and should not pacify congressional critics, since the ACS, like other professional societies, is designed to further the self-interest of its members rather than that of the American taxpayer. Indeed, it may properly be asked why the government should subsidize the activities of a wealthy professional special-interest group when that group discriminates against public libraries and other "non-members" through different prices charged for its publications and services.

Why was this large contract awarded without conducting a simple and inexpensive market survey to determine who needs the "product" and what they would be willing to pay for it? Whenever the chemical notation systems are discussed at scientific meetings most chemists appear uninterested. Yet, the same small clique of notation enthusiasts shows up—representing industry, government, and documentalists—with perhaps only a dozen or so persons seriously involved. This does not mean that the benefits of a chemical registry and structure searching system will not ultimately benefit thousands of chemists throughout the world, in the same way that mankind may benefit from a hundred million dollar particle accelerator even though only a few may use it. It is rather a question of national priorities.

Criticism also comes from private entrepreneurs in the growing information industry. Such critics ask why government agencies completely bypassed established government procedures in the procurement of this multi-million dollar "research" program. The question is made even more pertinent by the fact that the program is more development than research since the answers to many of the "questions" spelled out in such detail in this report were available years ago from studies of the files of the Chemical-Biological Coordination Center.

The project shows no respect for

well-established statistical sampling techniques. Apparently the project managers feel that the only way to test their system is to build it first and try it later. Having planned on the encoding of 200,000 chemical compounds, the project has done closer to 800,000 and appears to be headed, in the already signed follow-up contract, toward at least doubling that figure. Where does one stop before deciding the system works? The elemental composition of chemical compounds has not changed much since the CBCC analyzed them, and the CBCC frequency distributions are particularly pertinent since *Chemical Abstracts* (C.A.) plans to go back and pick up several million old compounds.

A number of small developmental studies have been conducted which seem useful, such as comparing magnetic tape typewriters to key punch machines and typewriters for the input of information on chemical structures to a computer. But no one at C.A. seems to have asked why one should spend from one to two dollars per compound to get a structure into the file when it could be done at lower cost using the already tested Wiswesser notation system or a similar system. If one has to draw a structural diagram in order to "encode," then this will be the same in either system. But this requirement happens to be built into the C.A. system because the abstractors have converted diagrams reported by authors into nomenclature. If the encoding were done directly from the original documents or from similar sources containing the structural diagrams, it would be absurd to expect to use character readers in the near future. If one has the structure or a systematic name, then presumably algorithms exist or can be created which would convert systematic notations to connection tables or any other atom-by-atom connection one needs or desires. C.A. is almost obsessed with the objective of producing these connection tables as a by-product of a structure drawing operation. All this seems irrational if *Chemical Abstracts* is to be the source of the information.

The fact is that structural diagrams or their equivalents must exist in the original documents that will be retrieved in the registry system. There-

fore, much of the activity described in this report is in a vicious circle. Given the structural diagram, it appears to be economical, according to notation enthusiasts, to create a line notation. If the diagram is not given, then a name must be given; else where is the compound reported? Given the chemical name, one could also create algorithms for converting nomenclature to whatever canonical forms one desires. That the present report skirts these fundamental issues derives from the political nature of the controversy that has raged between the C.A. Dyson-International Union of Pure and Applied Chemistry enthusiasts and the Wiswesser enthusiasts. The situation would seem less political if the National Science Foundation or the Office of Science and Technology simultaneously had supported studies by both camps but, given the blessing of a National Academy of Sciences ad hoc committee that C.A. is the logical organization to "do" chemical notation things, accompanied by a government obsession to centralize all chemical information activities somewhere, the result was inevitable—a unilateral approach which is subject to many serious scientific and political objections.

Finally, the free dissemination of scientific information is already in jeopardy through the structure of this C.A. project. An important appendix discusses the means by which C.A. intends to retain the confidential nature of data included in its files. In addition to data on compounds reported in *Chemical Abstracts* itself, data from such government agencies as the National Cancer Institute and, one presumes, also the chemical-biological warfare branch of the Army, among others, will be tied into this system, but the compounds will not be accessible to those who do not have a right to such information. In brief, the premises of C.A. will now contain information which will be rated as "confidential," and one wonders when security guards and clearance procedures will be instituted in the new \$8 million C.A. edifice at Columbus, Ohio, for Chemical Society members and "non-members" alike.

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